the petals of flowers. Let us take the white lily. If the petal of the flower had been merely a sheet of thin glass you would not have seen that white colour. There would have been a little light reflected from the first surface and the back surface, but the petal is really composed of a vast assemblage of little cells, at each of which partial reflection takes place, so that it resembles some finely-powdered glass, which would form a white powder, because each little surface is capable of reflecting the light, although a single sheet of glass would not be white. The petal of the white lily is just in the condition of the powder. It is full of little cells, full, optically speaking, of irregularities, from each of which a portion of light is reflected, so that, all kinds being reflected alike, and there being nothing in the white lily to cause preferential selection of one over the other—nothing to sift the light, as it were—you get a considerable quantity of light reflected back to the eye, but it is white. What is the difference between that and the red poppy? The red poppy is, as it were, a white lily infused with a red fluid; there is light continually reflected backwards and forwards, just as before, at the surface of the cells; but that light, in going and coming, passes through the coloured juice of the plant. It is the same thing with a green leaf. The structure is irregular, optically considered; there are constantly reflections, backwards and forwards, of light, which penetrates a little depth and is reflected, and has to pass through a certain stratum of this colouring matter, to which the name chlorophyll has been given, but which is really a mixture. That is what takes place generally as regards the coloration of bodies; it is a phenomenon not of reflection, not of selection of one kind of light for more copious reflection than another, but of absorption, or the swallowing up of certain kinds of light. Reflection comes in in order to enable us to see the light which otherwise would not enter the eye at all, but would go off in another direction."

J. NORMAN LOCKYER

COSMIC METEOROLOGY

SEVERAL articles have appeared at different times under the title of "La Météorologie Cosmique," from the pen of M. Faye: the last and most complete forms an exceedingly interesting "Notice Scientifique," appended to the *Annuaire* of the Bureau des Longitudes for 1878. In this memoir, written with the usual clearness and talent of the distinguished French astronomer, a number of results connected with real or supposed solar, lunar, and planetary actions on our earth are examined and criticised. As M. Faye has omitted several facts of considerable importance in his "Notice," and has misunderstood others, a reconsideration of some of the questions he has studied, with the additional light to be obtained from the facts alluded to, may not be without interest and use.

M. Faye's thesis is given in the first words of his article "Meteorological phenomena have their origin in solar heat." It is added, "This is now no longer sufficient. Cosmic influences are introduced, those of the planets, of the spots and rotation of the sun, of shooting-stars, the moon, and besides these, magnetic and electric actions are supposed to intervene incessantly between the bodies of the solar system." I shall refer to some of the most important questions under their different heads.

The Moon's Influence in producing Atmospheric Variations.—The popular beliefs in the moon's influence on the weather are first disposed of; they are conclusions from unrecorded observations where the coincidences are remembered and the oppositions are forgotten; and they are opposed to strict deductions when all the facts are employed.

Agreeing, as all men of science do, with this decision, the question remains, Whether the moon may not have some slight effect in producing meteorological variations? She reflects, absorbs, and radiates the solar heat; may this heat, in accordance with the thesis, not produce some effect on our atmosphere?

Sir John Herschel had observed the tendency to disappearance of clouds under the full moon; this he considered a fact which might be explained by the absorption of the radiated lunar heat in the upper strata of our atmosphere. He cited Humboldt's statement as to the fact being well known to pilots and seamen of Spanish America. I may add the testimony of Barnardin de St. Pierre, who, in his "Voyage à l'Ile de Réunion," says: "I remarked constantly that the rising of the moon dissipated the clouds in a marked way. Two hours after rising, the sky is perfectly clear" ("Avril, 1768"). Herschel also cited in favour of his "meteorological fact," a result supported by the authority of Arago, that rather more rain falls near new than near full moon.

Arago's conclusion that the phenomenon was "incontestable of a connection existing between the number of rainy days and the phases of the moon" was founded on the observations of Schübler, of Bouvard and of Eisenlohr, three series which, on the whole, confirmed each other. Schübler also, as Arago showed, had found that the *quantity* of rain which fell was greater near new than near full moon. These results, accepted by Arago, have not been noticed by M. Faye when he cites Herschel only, as one of those "men of science who interest themselves in popular prejudices, take bravely their defence in hand and exert themselves to furnish not facts but arguments in their favour." It seems, indeed, to have been forgotten that Herschel's argument was given to explain what he considered a meteorological fact.

M. Faye founds his argument wholly on the conclusions of M. Schiaparelli from a weather register kept at Vigevano by Dr. Serafini during thirty-eight years (1827-1864).[1] The Italian physician entered the weather as clear, cloudy or mixed (*misti*), or rainy from morning to evening. M. Schiaparelli finds from this register that the sky was clearest in the first quarter of the moon. It has not been remarked that if the moon's heat has any effect in dissipating clouds, as Herschel and others believed, this must be seen best when the moon is near full, that is to say, during the night hours, for which Dr. Serafini's register has nothing to say. In confirmation of the conclusion that the moon does not dissipate the clouds, another result from the Vigevano weather register is cited, namely, that the greatest number of rainy days happens near full moon. This result is opposed to that derived from the observations of Schübler, Bouvard, and Eisenlohr.

The value to be given to observations of the number of rainy days must evidently depend on whether the observations include the rainy nights; and an investigation on this question, to have any considerable weight, should depend rather on the measured rainfall than on the term "rainy day," for which no distinct definition is given.

The great objection to M. Faye's conclusions, as far as the facts go, is to be found in their entire dependence on the Vigevano weather register (*da mane a sera*). No notice is taken of other observations and results showing a lunar action on our atmosphere, such as those already mentioned, which Arago considered incontestable, those of Mädler and Kreil, and the more recent investigations of Mr. Park Harrison and Prof. Balfour Stewart. All of these, and many others, must be carefully considered before we can accept the conclusion that the moon has no influence on our atmosphere. The subject is, however, too large to be entered into here at present, and it will be possible to study it better after other conclusions of the learned French Academician have been examined.

There is, however, a part of the argument, whatever the results obtained may say, which merits particular consideration; and that is, that the moon's *heat* cannot produce the phenomena in question. M. Faye shows that if the moon's reflected heat is in the same proportion as the reflected light, such heat cannot produce a change of temperature of a thousandth of a degree Fahrenheit. I would remark that Lord Rosse's carefully-made experiments with the most delicate apparatus have shown for the total heat radiated and reflected, nearly ten times the proportion given by the reflected light; but, as M. Faye observes, if the proportion were increased a hundredfold the effect would still be insensible. "How then," it is added, "can we expect such an action to dissipate the clouds when that of the sun does not always succeed?"

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[1] *Memorie del R. Istituto Lombardo*, L. 10.

If, however, we can establish that real lunar actions exist which cannot be explained by the moon's heat reflected or radiated, the only philosophical conclusion will be that the moon must act in some other way, which it will be in the interests of science to seek out.

Influence of the Sun-spots on the Earth's Magnetism.—It has been found that since the first accurate series of magnetic observations, towards the end of last century, to the present time, the maximum and minimum of sun-spot frequency have occurred at the same times as those of the diurnal oscillation of the magnetic needle; but because Dr. Wolf has believed that in the interval between 1787 and 1818, when the observations of both phenomena were very incomplete, there were only two cycles for both, and because Dr. Lamont and myself believe there were three for both, M. Faye concludes that the mean length of the period for the magnetic needle was different from that for the sun-spots. The true conclusion is—if Dr. Wolf is right the mean length of the period for *both* phenomena since 1787 is nearly twelve years; if the other view is right the mean length for *both* is 10·45 years. I have already considered this question in NATURE (vol. xvii. p. 262). The *egalité rigoureuse* of the lengths of the periods sought by M. Faye is thus established whatever view be taken.

It is next sought to show that any variation in the earth's magnetism in the decennial period cannot be due to variations in the solar heat produced by the spots. Founding on Mr. Langley's observations for the temperature of the photosphere, of the nucleus and the penumbra of spots, M. Faye shows, making use as before of the absolute temperature of the earth, that the variation of temperature due to the spotted surface of the sun cannot exceed ± 0°·3 Fahr.

I venture here to remark, in the first place, that I have given strong grounds elsewhere for believing that the sun-spots are not the cause of the decennial period in the magnetic variations; but that both are due to the action of a common cause.[1] I quite accept then M. Faye's conclusion. It may, however, be argued that more heat proceeds from the sun in years of many, than in those of few, spots, but the observations of temperature, which have been made with so much accuracy in many countries for the last twenty years, will prove at once that no marked variation of the mean temperature occurs in the decennial period.

We must here, however, consider the reasoning which has been employed on this subject. It is shown that the variation of temperature due to the spotted surface of the sun cannot explain the change in the amplitude of the diurnal oscillations of the needle. It is, however, a mere assumption that this oscillation is due to the solar heat, an assumption for which there is really no sound basis, unless some very rude attempts at a hypothesis can be so considered.

M. Faye says, "This phenomenon is absolutely general; it appears over the whole earth following the same laws, only the amplitude of the oscillation, which in the mean is at Paris 10′, is reduced to 1′ or 2′ between the tropics, and goes on increasing thence towards the poles." This variation of range is afterwards compared with the inverse law for the diurnal variation of the barometer, the range of which *diminishes* from the tropics towards the poles.

This view, however, is founded on a misconception of the facts; we might just as well say that the earth's magnetic force diminishes from the equator towards the poles (which is just the reverse of the truth), because its horizontal component does so. The mode in which this movement varies in amount from the tropics towards the poles appears to be imperfectly known, and as it is essential to make this clear before we can compare the facts with any hypothesis, I shall now attempt to do so.

Diurnal Variation of the Magnetic Declination.—The horizontally suspended magnetic needle performs an oscillation in twenty-four hours, during which the north end is most westerly in the northern hemisphere, and most easterly in the southern hemisphere, between 1 and 2 P.M. If we consider needles at different stations on the same meridian, it might be supposed that as we approached the equator this opposition of movement would result in the needle becoming stationary. That was Arago's conclusion. This idea, however, neglected the position of the sun. The range of the oscillation is greatest in our hemisphere when the sun is north, and greatest in the southern hemisphere when the sun is south of the equator. It is only when the sun has the intermediate position, near the time of the equinoxes, that the forces are nearly balanced at equatorial stations.

If we suspend an unmagnetic steel needle horizontally on its centre of gravity, so that it can move both in a horizontal and vertical plane, and then magnetise it, the needle *dips* with one end below, the other above, the horizontal plane passing through its centre; and the direction in which it lies is that of the earth's magnetic force. If this needle moves up, or down, or sideways, this is because other forces pull it one way or another, or because the direction of the earth's magnetic force has changed. Let us suppose in the first instance that the latter hypothesis is the true one, that is to say, that the earth's magnetic axis moves with the sun, the north pole having the greatest movement when the sun is in the northern hemisphere. If the dipping needle moves in the plane perpendicular to the vertical plane, through an angle u, and we wish to know what would have been the corresponding movements eastwards or westwards, if the needle had been made horizontal, by the addition of a small weight to the end above the horizontal plane, we must *divide* the angle u by the cosine of the *dip* below the horizon. Now in England the cosine of the dip is about $\frac{1}{3}$, so that the horizontal needle would have moved through an angle of $3u$. We obtain similarly the former from the latter by *multiplying* by the cosine of the dip.

We may now see, from observations made at different stations, what is the range of the monthly mean diurnal variation of the horizontal needle multiplied by cosine *dip* in a month for which it is a maximum in the northern hemisphere. The following are approximations to the mean ranges thus obtained from several years observations in the month of August for the northern hemisphere.

Station.	Lat.	Dip.	Range of hor. needle × cos. dip.
Makerstoun	56° N. ...	71° N. ...	4′·0
Toronto	44 ,, ...	75 ,, ...	3·5
Simla	31 ,, ...	42 ,, ...	4·6
Bombay	19 ,, ...	19 ,, ...	5·4
Madras	13 ,, ...	8 ,, ...	4·8
Trevandrum	9 ,, ...	3 S. ...	3·6

In a similar way we find for the month of February for southern stations—

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[1] "On the Decennial Period" (*Trans.* Roy. Soc. Edinb., vol. xxvii. p. 593).

St. Helena	16° S.	...	22° S.	...	4′·8
Cape of Good Hope	34 ,,	...	53 ,,	...	4·6
Hobarton	43 ,,	...	70 ,,	...	4·3

It will be seen that in the months in which the sun's action is a maximum,[1] for each hemisphere the east and west movements of the needle in its true position (that which is independent of gravity) do not vary with latitude; the maximum range appears, in fact, to take place near the tropics, and when the sun is in the zenith.

If, however, we should prefer to consider the oscillations of the horizontal needle due to the direct action of electrical currents upon it rather than upon the earth, we must remember the change of the force which directs the needle as we proceed from one latitude to another. If we wish to compare the vertical heights through which a body will fall in a second of time on an inclined plane at two stations, we must take into account not only the force of gravity at each station, but also the angle which the inclined plane makes with the horizontal plane. When we employ the same unit of directive force at the stations in the north hemisphere for the month of August, we obtain the following comparative ranges:—

Makerstoun ...	4·3
Toronto ...	4·8
Simla	4·4
Bombay ...	4·6
Madras	3·8 (September = 4·2).

The values are less for Trevandrum and St. Helena, but there is no appearance of a law which can be referred to latitude; and there is no way in which we can examine the question which will satisfy such a relation. If we take any zone or zones of the earth which will include as much of the northern as the southern hemisphere, the mean movement for them will be nearly zero, on account of the opposite directions of the oscillations; it is for this reason that there is a diminution of range, especially in the months near the equinoxes, for equatorial stations.

On the whole the conclusion is, that the diurnal law of oscillation east and west of a magnetic needle is nearly the same in all latitudes for a given position of the sun and a given directive force. The deviations from this rule are connected with magnetic disturbances which have most effect near the poles, and with the opposition of forces near the equator. We have thus to deal with a phenomenon which is little dependent upon local causes, and which may, in its great features, be considered cosmic.[2]

These facts understood, we are now in a better position to consider the great change in the range of this oscillation, which occurs in the decennial and sun-spot period. Let us examine what that change really is. It does not matter here whether we refer to the motion of the horizontal or of the dipping needle; we find that if the mean range is counted 10 in England in the years when it is a minimum, the years of fewest spots, then it becomes 16 or thereby when it is a maximum, that is, in the years for which the sunspots are most numerous.

Now this great change in the effect of the solar action is felt in nearly the same way, and to the same proportionate amount, all over the globe. The law of the oscillation is not changed; the needle attains the most westerly position in one hemisphere, and the most easterly in the other, at the same hour as before, but the oscillation is nearly sixty per cent. greater at Hobarton in Van Diemen Island, at Trevandrum on the magnetic equator, at Toronto in Canada, and in England.

When the observations have been continued sufficiently long with equal care, we can find that the ratio of the maximum range to the minimum is undergoing, at some stations at least, a slow change. Thus, at Trevandrum the successive ratios of the maximum of 1860 and 1870 to the preceding and following minima are—

$$\frac{1860}{1856},\ 1\cdot59;\quad \frac{1860}{1866},\ 1\cdot57;\quad \frac{1870}{1866},\ 1\cdot55;\quad \frac{1870}{1877},\ 1\cdot51.$$

It must be remembered that the thesis which M. Faye supports is, that the diurnal oscillations of the magnetic needle are due to the solar heat, and that he has shown that no appreciable change of temperature is due to the spotted surface of the sun. We may ask then, Where are we to find the change of temperature which causes so great a variation in the sun's action? We need not calculate the difference of temperature between the photosphere and the nucleus of a spot, and we need not theorise on the possible difference between the solar radiation when there are few and many spots; we have got thermometers; we have even observations of the evaporation of water, from which solar action M. Faye finds the atmospheric electricity which should produce the magnetic variations. What do they say? If effects have any relation whatever to their causes, surely when the effect of the solar action in producing the diurnal variations of the earth's magnetism is increased by a half or three-quarters of its value from the time of most spots till the sun shines with unspotted surface, we should expect some marked changes of temperature from year to year, if change of temperature is in question. I have already remarked that no such change of temperature has been found.

M. Faye says with reference to the question, What is the cause of the decennial magnetic variation? "The question would perhaps be embarrassing if we had only that to ask, but the elements of terrestrial magnetism present other variations which are as much independent of the sun-spots. Such are the secular changes which displace gradually the magnetic poles of our terrestrial globe. We must seek the cause not in the heavens but in the slow modifications of which the earth's surface is the theatre. They are due probably to the works of men and above all to the continued action of geological forces." Of course these ideas refer to the secular changes, even for which they will not be readily accepted, but they do not touch on the fact that an explanation is offered, that of the solar heat, for the diurnal variations, and that no evidence is produced that the supposed cause undergoes any change in the decennial period.

I have referred only to oscillations of the magnetic needle, which may be considered due to the variations of an easterly force; but the force of the earth's magnetism which directs the needle north and south, obeys also a law of diurnal variation, and the range of this variation follows the same law in the decennial period as that for the ranges of the oscillations. Thus at Trevandrum, near the magnetic equator, the *range* of the diurnal variation of the total force of the earth's magnetism in the year for which it was a maximum, was to the range in the year of minimum as 17·7 to 10. The corresponding ratio of the ranges for the oscillations of the needle having been as 15·9 to 10.[1]

As there is not the slightest evidence of a decennial variation in the solar heat, and as there is an absolute certainty that if any variation exists it is of an amount so very small that it could not account for the great changes in the magnetic variations, the conclusion appears to me inevitable, that these variations are *not* due to the solar heat. We fortunately possess the means of deciding this question by the study of phenomena due to our satellite, whose heating action M. Faye has shown to be quite inappreciable.

JOHN ALLAN BROUN

(*To be continued.*)

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[1] The month of maximum varies within the tropics: at Madras it is in September, and for that month the range multiplied by cos. dip = 5′·3 nearly.

[2] For these reasons no such inverse relation with latitude exists as M. Faye has supposed between the diurnal oscillation of the magnetic needle and of the barometer.

[1] I would remark here that the epoch of maximum range of force was not exactly the same as that for the maximum oscillation, a fact which may not be without importance when the mode of solar action is investigated.